TO: **Andrew Christensen, Chair, Space Science Advisory Committee**

FROM: **Jonathan I. Lunine, Chair, Solar System Exploration Subcommittee**

SUBJECT: **Solar System Exploration Subcommittee Meeting**

The Solar System Exploration Subcommittee (SSES) of the Space Science Advisory Committee (SScAC) met October 23-24, 2003 at NASA HQ. The purpose of this memorandum is to summarize the findings of that meeting and ask SScAC to consider them and transmit its recommendations to Mr. Orlando Figueroa, Director of the Solar System Exploration Program and Acting Director of the Mars Program.

Discovery:
The defining quanta of the Discovery Program are PI-led missions, competitively selected every 18-24 months to address focused science within a moderate cost cap. For example, for the next AO opportunity development costs are capped at ~$360M in FY04 dollars, including launch vehicle. This mission cap enables the Discovery line to sustain a program with frequent flights at an annual cost of approximately $250M. With 3 missions operating in space, 2 more in development, and 2 currently in formulation, Discovery is appropriately viewed as a highly successful program and has served as a model for the New Frontiers Program (NF), recently initiated with the New Horizons mission to Pluto and the Kuiper Belt (see below). In-depth exploration of the solar system, with its diversity of objects and environments, requires a multi-mission approach of this type.

NASA's third and fourth Discovery missions (Stardust and Genesis) are proceeding as planned - Stardust has successfully executed two samplings of interstellar dust and one sampling of asteroid material. Its final sample (comet dust) will be in January 2004. Genesis has successfully sampled the solar wind for 22 months - critical for understanding isotope ratios in primitive meteorites - and will return to Earth in September 2004. The program has issued its first Discovery data analysis program AO.

The SSES commends Solar System Exploration Division scientists and engineers for their work on a number of current Discovery issues, leading to a situation in which the development of Deep Impact, Messenger, and Dawn appear to have ways to get back on track. The committee is particularly pleased that the Division is aggressively pursuing the implementation of the recommendations made by SSES and SScAC to ensure that Discovery missions continue to be successful and stay within the prescribed cost cap.

HST
As HST reaches the end of its planned operational lifetime, and the JWST is developed to replace it, the Physics and Astronomy Division (PAD) has developed an extended mission plan using one shuttle mission, SM4, to repair systems and install new instruments. A panel of experts convened by Congress supported this plan. A subsequent committee (“The Bahcall Committee”) chartered by the PAD recommended adding another repair mission (SM-5) to the Space Shuttle manifest so as to further extend the

lifetime of the mission and possibly add more new instruments. In its report, this most recent committee suggested that the SM5 mission be funded in an open competition with the Explorer and/or Discovery lines of low cost missions. SSES is opposed to the use of the Discovery line as a means of funding the SM5 suggestion, either through an open competition or other means, for reasons given below:

1. The Solar System Decadal Survey states the following: *"Given Discovery's highly successful start, the SSE Survey endorses the continuation of this program, which relies on principal-investigator leadership and competition to obtain the greatest science return within a cost cap. A flight rate of no less than one launch every 18 months is recommended."* Under the prioritized list of flight missions in the decadal strategy, the Discovery flight line is ranked first in the small mission category. The Bahcall committee ruled out enabling the SM-5 mission by altering or canceling missions or programs given high priority in the Academy's Decadal Surveys. In particular, it states: "…no already approved science project would be adversely affected. It is our intention that this process should maintain the relative priorities of the Decade Surveys". This quite appropriate posture appears to be inconsistent with its own specific recommendation to impact the Discovery mission *line*, which is the highest priority effort in the area of small missions in the Solar System Decadal Survey.

2. The cost of the SM-5 mission (estimated by the PAD to range between $0.6-1.2 billion plus launch, depending upon inclusion of new instruments) would require the equivalent of between 3 and 5 Discovery missions as measured in the same way (i.e., excluding launch costs, which normally are included in pricing Discovery missions). Expressed another way, if SM-5 were implemented using the Discovery budget it would put the Discovery program on hold for a period of between 5 and 7 years. It was suggested that some single, comparably scaled competitor could be proposed; however, we found this suggestion unrealistic for several reasons. The selection criteria and ground rules by which the suggested competition would proceed were unclear, and the "related science goals" were not stated. The likelihood of actually providing HST with new instruments *via* SM5 is unknown, so the scope of candidate SSED competitors remains ill-defined. A mission of full-up SM5 scale is not in fact a Discovery mission. A hallmark of Discovery missions is that they are led by scientific PIs, not NASA centers, and provide the opportunity to train future generations of scientists. It was partly on these grounds that the line was approved by Congress, and divergence from this philosophy could lead to the loss of the entire program. Even were it not to do so, a 5- to 7-year program suspension would put any future Discovery missions beyond the horizon of the Solar System decadal plan. It could also lead to an even longer flight hiatus, as PI expertise and hardware lines for Discovery might be lost and would have to be ramped up again at the end of this period. Finally, changing from a multi-mission to a single-mission philosophy harms SSED's ability to explore diverse solar system targets.

Overall, in our judgement, the cost to SSED mission diversity and science return would far outweigh any possible benefit from a currently ill-defined SM5 mission to extend HST, if that were accomplished using Discovery resources (deemed high priority by the

Decadal Survey) as proposed by the PAD's *ad hoc* committee. We support the PAD plan to extend and reequip HST with their own resources, using an SM4 mission.

New Frontiers Program

The New Frontiers (NF) Program fills the need for a larger, intermediate class of PI-led missions, prioritized and recommended by the Solar System Decadal Survey of the NAS. NF is off to an exciting start, with the recent issuance of an AO for the four Decadal Study (DS) mission concepts remaining after New Horizons. The NF AO also provides for the ongoing review and selection of at least some extended missions - a need long recognized by NASA and the SSES. NASA has begun to plan how to regroup around DS science concepts which remain unselected, for whatever reasons, at the outcome of the AO selection process. For instance, it is foreseeable that either (a) while all relevant technology is mature, one or more of the DS mission designs will be found to be unachievable within the NF cost cap, or (b) one or more of the DS mission designs will be absent from the responses because of real or perceived immaturity of enabling technology.

A feasible response to (a) would be, as proposed by NASA, to constitute Science Definition Teams (SDTs) with the charge to refocus missions which are too expensive on some highest priority subset of the DS science goals. We are concerned that existing, unconnected technology development programs may not, in fact, be adequate for responding to (b). Some aspects of Code R's "Technology for Extreme Environments" program might be applicable starting in FY05, but there is no method for determining if these are the most important technologies for NF, for providing guidance on their progress, or for complementing them with new efforts as needed by NF. A solution would be to establish technology development teams (TDTs) within NF with the specific goal of bringing enabling technology for the unselected DS mission profiles to maturity.

Overall, we suggest that NASA develop a plan to constitute Science Definition teams and Technology Definition Teams (SDTs and TDTs), working within the NF program, to refocus science goals and advance immature technologies for unselected DS missions. Having such a plan in advance may even allow for a more satisfying conclusion to the AO process.

Mars

MSL: SSES shares concerns expressed by the Mars Exploration Program Analysis Group (MEPAG) regarding growing threats to the ability of the Mars Scientific Laboratory (MSL) to accomplish its scientific objectives within budget. Not only does MSL face the typical cost growth that occurs during pre-phase-A as the mission technical design matures, but its proposed use of nuclear power raises the possibility that it must comply with category IVc planetary protection criteria, which will require significant additional funding.

It is encouraging that the instruments needed to make critical measurements for MSL appear to be in development in the various technology programs (e.g., MDIP and ASTID). However, the integration of even mature instruments and sample handling devices into the needed package is not trivial and will require significant funding. There are concerns, for instance, that even a fully and thoroughly sterilized spacecraft could contain enough bioload to compromise investigations that "follow the carbon" in the Mars environment. The SSES recommends that MEPAG proceed with its plans to address this issue in more depth, in collaboration with the MSL project.

As MSL tries to balance expectations and resources, it is important that the mission continue to meet or exceed the minimum science floor defined by the Project Science Integration Group (PSIG) in terms of mobility, lifetime, and payload, including the acquisition and proper analysis of samples. While it may be tempting to return to a solar powered mission to constrain costs, including those associated with planetary protection, the SSES worries that the resulting mission would be compromised in its ability to carry out the required program of scientific measurements at higher latitudes, the region of greatest scientific interest for astrobiology.

Post-MSL: The Next Decade plan, prepared by the Mars Science Program Synthesis Group (MSPSG), which was chartered by NASA and included MEPAG members, program engineers, and advance planning engineers, is an excellent response to the OMB challenge to define a post-MSL Mars program. The plan proposes four "pathways" for Mars exploration in which a particular sequence of missions would respond, within a Mars Exploration Program funded at the current level (plus inflation), to specific major discoveries by near-term missions. The SSES endorses the view that the order and timing of major missions for the next decade (post-MSL) should build on the discoveries of the current program of Mars exploration.

However, the SSES also resonates with the concern expressed by MEPAG that the Mars exploration program may become too limited in scientific scope. In the current pathways plan, the broader understanding of Mars geology, atmosphere, and geophysics is a primary objective of the major missions only if all avenues for exploring habitable environments are exhausted. Network and future orbiter missions (e.g., aeronomy) are identified only in that pathway and only then after sample return. Thus, the SSES believes that the AO's for Scouts should not dictate specific science roles in particular pathways, but rather continue to allow proposers the freedom to develop missions with a broad scientific and technical scope.

The present Next Decade plan also assumes that the "best" sites for exploration after MSL can be derived without further orbital reconnaissance and "ground truth". While many good sites will surely be identified, the SSES worries that there remains a significant risk that a sample return or astrobiological robotic laboratory might be directed to a nonproductive site. None of the pathways has a mission examining many sites, although the challenge would be to do so with appropriately powerful in situ instruments. SSES intends to explore this complex issue in more detail in future meetings.

PDS

Progress on the Planetary Data System (PDS) appears to be satisfactory, and will be of significant value to the planetary community in managing the increasing large data sets produced by current and planned missions. Our discussions on this topic suggest that current NASA plans may not be sufficiently aggressive in developing an integrated data system and associated tools that would permit a larger group of scientists, including those without direct mission experience, to utilize these data sets. Scientists in other disciplines, ranging from paleobiology to molecular biology, have discovered that the effort required to develop this technology is well worthwhile. By increasing the ease of access to such data, scientists spend less time managing data and more time answering questions to which the data can be applied. Further, the availability of data in such formats will encourage new scientists to enter the field; this generally has substantial positive impacts. Experience with some large data systems (e.g., the early EOSDIS) indicates that care must be taken to ensure that this development does not get out of hand. PDS should first give emphasis to basic capabilities. Capabilities beyond this need not--perhaps should not--be developed by PDS itself, but instead could be developed as the result of a peer-reviewed competition.

Instrument development (other than Mars)

The committee is concerned that Code S, leaving aside the Mars and astrobiology programs, does not have an ongoing program to support instrument development through the mid-TRL levels (i.e., beyond breadboards up to flight demonstration). The PIDDP program as currently constituted is charged with supporting instrument definition and development only through breadboard level. The lack of a mid-TRL instrument program means that certain measurement concepts are rejected out of hand during mission definition, or that added risks are assumed when a mission with new instruments is selected for development. Instrument development risks are often important or dominant contributors to overall mission risk. The cost-capped mission program lines, in particular, would benefit from a mid-TRL instrument development program, and the Committee would strongly support mission risk reduction via such a program.

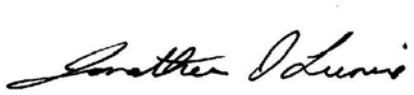
Astrobiology

SSES congratulates Dr. Bruce Runnegar of UCLA on his selection as the new Director of the NASA Astrobiology Institute and wishes him well as he takes the helm from Acting Director Dr. Rosalyn Grymes, who has ably led the institute since the departure of Dr. Barry Blumberg. With the recent recompetition the NAI has moved into a phase of maturity in which new institutions and new programs are being included. The institution of the focus groups and extensive field programs are the most recent notable and positive accomplishments of the NAI. The SSES notes that, given the increasing number of field expeditions involving sample collection under the partial aegis of the NAI, a plan needs to be developed for the curation, distribution and dissemination-of-information-on the

field samples. The cycle of intense competition among teams, followed by a 4-year cooperation including sharing of data and personnel, followed by another intense completion, constitutes a sociological experiment not explicitly considered in the management of the institute and –for example—the implementation of the virtual institute technology. It is essential that the NAI director, and NASA itself, follow the impact of these features of the NAI on scientific productivity and education of students in astrobiology. With regard to the long-term future of astrobiology, SSES raises two issues: (1) Is the balance between the small core of co-investigators, whose research is at least partially funded through NAI, and the halo of collaborators, with minimal or no NAI funding, appropriate for the growth of this nascent field toward critical mass? (2) Is there a proper balance in funding among NAI, R&A, NSCORTs and technology development programs? SSES intends to examine these issues in more detail in the future.

Finally, let me close by expressing the committee's best wishes to Colleen Hartman as she departs NASA and pursues new challenges elsewhere in the Federal Government. Her superb abilities as Solar System Exploration Division director will be missed, but the committee is very pleased that Mr. Orlando Figueroa has been appointed to this position. His able leadership in the Mars Exploration Program during a crucial period in which the MER's were prepared and launched has been key to that program's forward momentum. We wish him well in his new position, and urge the Office of Space Science to move ahead as soon as possible to permanently fill the Mars Exploration director's position.

With best regards,



Jonathan I. Lunine
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[1]Space Studies Board, Assessment of Mars Science and Mission Priorities, National Academies Press, 2003.